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STRUCTURAL EMPIRICAL EVALUATION OF JOB SEARCH MONITORING*

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To evaluate search effort monitoring of unemployed workers, it is important to take account of post-unemployment wages and job-to-job mobility. We structurally estimate a model with search channels, using a controlled trial in which monitoring is randomized. The data include registers and survey data on search behavior. We find that the opportunity to move to better-paid jobs in employment reduces the extent to which monitoring induces substitution toward formal search channels in unemployment. Job mobility compensates for adverse long-run effects of monitoring on wages. We examine counterfactual policies against moral hazard, like reemployment bonuses and changes of the benefits path.

1. INTRODUCTION

Generous unemployment benefits schemes are potentially subject to moral hazard: Unemployed workers reduce search effort and increase their reservation wage. This may reduce their transition rate to work. Policymakers have become interested in approaches to counteract this by using alternative policy measures (e.g., OECD, 2007). The monitoring of job search behavior (including the threat of punitive benefit reductions) and the payment of reemployment bonuses are examples of such policy measures. These measures, as well as the simple policy device of lowering unemployment benefits, have the disadvantage that they tend to reduce the quality of the post-unemployment jobs (e.g., Acemoglu and Shimer, 2000). The relative merits of the various policy measures can only be assessed empirically. In the absence of a large range of randomized controlled trials, such an assessment involves counterfactual evaluations.

This article provides a structural analysis based on a randomized social experiment of a monitoring scheme for unemployed workers. Structural analysis aims at uncovering policy-invariant parameters characterizing individual preferences and boundary conditions resulting from labor market imperfections and policy constraints. Our data include post-unemployment outcomes like wages and job durations. The observation of such outcomes is important because it enables us to address the extent to which policies against moral hazard have detrimental effects on them. Moreover, it enables us to address the importance of two tools that the individual has at his disposal to mitigate the reduction of his expected present value that these policies cause. First, the individual may substitute search effort away from the search channels that are not monitored to the channel that is monitored. Channel substitution may be beneficial from the individual point of view, and it may help to prevent a low-quality job match, but it tends to reinforce the moral hazard problem. The second tool is on-the-job search. Job-to-job

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transitions reduce the importance of the first job accepted after unemployment. A low starting wage can be mitigated by subsequent wage gains. On-the-job search therefore reduces adverse post-unemployment effects of policies aimed at fighting moral hazard, while at the same time it does not stimulate moral hazard during unemployment. The structural model we develop is consistent with the differences in behavior and labor market outcomes between the treatment and control groups observed in the data.

By structurally estimating a model that distinguishes between different search channels and that allows for job-to-job mobility, we can quantify the relative importance of the two above-mentioned mechanisms. Moreover, we may study their interaction. We show that the extent of channel substitution depends on job mobility. If it is easy to move to better jobs while in employment then channel substitution is less strong than otherwise. This is because with a high job mobility, any job with a low wage can be exchanged quickly for a better job, so the attractiveness of finding work using all available channels is high. On the other hand, a high option value of job mobility may decrease the incentive to make search efforts among the unemployed. If a high job mobility does go along with a high search effort by the unemployed, then, if actual job mobility is high, the imposition of a minimum threshold for the monitored search effort level will more often not be binding. All this suggests more in general that it is relevant to take post-unemployment choices into account when evaluating the policy measures of the employment office. In the policy evaluation literature, the role of subsequent job mobility has typically been ignored.

The data concern a sample of relatively skilled individuals from the Netherlands. A subset of the variables in the data has been analyzed in Van den Berg and Van der Klaauw (2006). That study consists of a reduced-form analysis of the average treatment effect of the monitoring program on unemployment durations. It did address the issue of channel substitution, but the study did not have access to post-unemployment outcomes. The reduced-form results do not provide evidence for a strong effect of monitoring on the unemployment duration, but they do indicate that channel substitution takes place. Reduced-form studies cannot extrapolate such results to individuals in different circumstances. Structural analysis is more amenable to this. In our present article, we exploit the advantage of structural analysis that it enables counterfactual policy analysis (see, e.g., Eckstein and Van den Berg, 2007, for a more general discussion of the advantages of structural analysis, with a focus on unemployment outcomes).

Our study complements the empirical literature on the effectiveness of monitoring unemployed workers (see Johnson and Klepinger, 1994; Dolton and O'Neill, 1996; Gorter and Kalb, 1996; Klepinger et al., 2002; Ashenfelter et al., 2005; Van den Berg and Van der Klaauw, 2006; McVicar, 2008; Manning, 2009; and Micklewright and Nagy, 2010). All these studies provide reduced-form analyses, and many are based on randomized social experiments. The evidence is surveyed in Van den Berg and Van der Klaauw (2006). In general, the effect of monitoring is stronger if labor market conditions and job prospects are worse.

Evaluation studies on post-unemployment effects of "treatments" during unemployment include Ham and LaLonde (1996), who examine training programs, Dolton and O'Neill (2002), who examine a counseling and monitoring program, and Van den Berg and Vikström (2014), who examine punitive sanctions for individuals who do not comply with monitoring guidelines. The latter study finds that sanctions on average lead to significantly lower accepted daily wages. Finally, there is an expanding branch of literature that uses structural models to evaluate active labor market programs (Adda et al., 2009; Fougère et al., 2009; Wunsch, 2013; Lise et al., 2015; Cockx et al., 2018; Gautier et al., 2018).

Section 2 provides institutional details for the policy that is evaluated in the social experiment. It also describes the experiment itself. Section 3 summarizes our data. These are from a range of registers as well as from a survey among the participants in the experiment. In Section 4, we develop and analyze the theoretical job search model with multiple job search channels and job-to-job mobility. We discuss identification of the structural model and we derive the likelihood function. Section 5 presents the parameter estimates, the evaluation of counterfactual policies, and the effects in different labor market settings. We describe a range of sensitivity analyses in Section 6. Section 7 concludes.

2. UNEMPLOYMENT INSURANCE AND MONITORING POLICIES AND THE RANDOMIZED SOCIAL EXPERIMENT

2.1. *Unemployment Insurance.* In this subsection, we briefly describe the Dutch unemployment insurance (UI) system in the late 1990s, which includes our observation period. If a worker younger than 65 years becomes unemployed, she is entitled to UI benefits (provided that some conditions of her employment history are fulfilled). The entitlement period and level of benefits are determined by the worker's labor market history. Usually, the initial level of benefits equals 70% of the wage in the job previous to unemployment with a maximum of 138.84 euro gross per working day.[2] The minimum wage equals 49.12 euro gross per working day. The exact duration of the entitlement period for initial benefits lies between six months and five years (depending on the worker's employment history). After the initial entitlement to benefits expires, the unemployed worker receives extended benefits that cannot exceed 70% of the minimum wage.

According to the Unemployment Law, an unemployed worker has the following obligations in order to be entitled to UI benefits: (i) prevent unnecessary job loss; (ii) take actions to prevent from staying unemployed, so she has to search for a job and accept appropriate job offers, register as a job searcher at the public employment office, participate in education and training, etc.; and (iii) keep the local UI agency informed about everything that is relevant to the payment of the UI benefits. If an unemployed worker does not comply with these rules, the local UI agency is authorized (not obliged) to apply a sanction to that worker. In general, although the local UI agencies are mainly responsible for paying UI benefits, they also provide training and schooling. The public employment offices act as matching agents, not only to UI recipients, but also to welfare recipients and employed workers searching for (new) jobs.

2.2. *Job Search Monitoring.* The monitoring program that we consider has been a nationwide policy since April 1998. The program was targeted toward UI recipients with low expected unemployment durations, during their first six months of unemployment.

At the *intake meeting* of UI, any individual is classified ("profiled") into one of four "types," based on individual characteristics such as work experience, age, and education, and on some subjective measures such as expected job search behavior, flexibility, language skills, and presentation skills. Only those who are expected to have sufficient skills to find a job (Type I) are exposed to the monitoring scheme we consider, with the additional restriction that the UI eligibility period exceeds six months. In the inflow of unemployed workers into UI, 80% are classified as Type I, whereas in the stock of UI recipients, about 60% are classified as Type I. Excluded from the policy are individuals who know at the date of UI registration that they will start a new job within three weeks and Type I unemployed workers collecting short-period benefits. Exposure to the monitoring scheme lasts half a year. During this period, the unemployed workers have a meeting at the local UI agency every four weeks.

The *intake meeting* takes place within three days after the start of the payment of the UI benefits. The quality of application letters and the resume are examined, the different channels through which work can be found are discussed, and a plan is made about what the individual should do until the next meeting. Although the local UI agency can inform the unemployed worker about possible job entries, it is not allowed to act as an intermediary between unemployed workers and firms. Offering or pointing out specific vacancies to unemployed workers is the task of the public employment offices. During the intake meeting, it is emphasized that a positive and active attitude toward job search is warranted.

[2] Some individuals are only entitled to "short-term" benefits, which is at most 70% of the minimum wage for a six-month period, but these are not subject to the monitoring regimes we consider and are not in our data.

The *follow-up meetings* focus on efforts to apply to specific job vacancies and employers. During these meetings, the plans of the previous meeting are evaluated and a plan for the next period is made. If the unemployed worker did not comply with the plan, she may be punished with a sanction in the form of a reduction of the UI benefits. The average sanction for insufficient job search is a 10% reduction of the UI benefits for a period of two months. In addition, reports on search activities have to be sent in every week. Below, when we refer to "the" monitoring scheme or policy, we refer to the monitoring that goes beyond the processing of these handwritten weekly reports. This is because in the experiment, both treated and controls were obliged to submit these reports.

The monitoring scheme is inexpensive. The Dutch National Institute for Social Security pays the local UI agencies on average 152.46 euro per individual for monitoring. This is paid at the beginning of the UI entitlement period and does not depend on the realized unemployment duration. Each meeting includes a check on whether the unemployed worker is still eligible for UI benefits. Performing this check would otherwise cost on average 17.52 euro. So those costs should be excluded from the costs of the scheme, for each month that an individual collects UI benefits.[3]

2.3. *The Experiment.* The experiment concerns all Type I unemployed workers who started collecting UI benefits between August 24 and December 2, 1998, at the local agencies in two of the largest cities of one nationwide UI agency. In the remainder, we refer to these as City 1 and City 2. The inflow into UI at these local agencies is relatively large, and the agencies have a high reputation for carrying out monitoring activities in a highly orderly fashion. Both facts have played a role in the selection of these local agencies as venues for the experiment. All individuals who satisfy the policy eligibility criteria as listed in the previous subsection are included in the experiment.

During the UI intake meeting, the employee of the local UI agency establishes whether or not a UI recipient is eligible for monitoring. In case of eligibility, the unique ID number of the unemployed individual is instantly electronically transmitted to an independent computer center. This center does not know anything about the unemployed individual beyond the ID number. The center then decides based on a series of random numbers, which were realized by a random generator in SPSS before the start of the experiment, whether the unemployed individual is assigned to the treatment group or the control group. Notice that this is equivalent to a procedure where randomization takes place within the local agencies in both cities. Here, "treatment" refers to the exposure to the actually implemented monitoring policy, whereas "control" refers to the absence of this exposure. Individuals selected in the treatment group have to show up at a monitoring intake meeting within three days and subsequently in the follow-up meetings that take place every four weeks. The unemployed workers in the control group only communicate with the local UI agency by way of sending in written forms stating the current status of their job search activities.

After the first six months of collecting UI benefits, the monitoring ends for individuals in the treatment group. All individuals who are still unemployed after six months thus end up in the same regime that may involve alternative active labor market policies.

The participants in the experiment are not informed about the occurrence of the experiment. In the absence of the experiment, all of them would be subject to the monitoring treatment. None of the individuals in the control group in the experiment complained about a lack of monitoring. The setup ensures that the data do not suffer from initial nonrandom nonparticipation/noncompliance in the experiment, and participants cannot leave the experiment for any reason other than stopping collecting UI benefits.

[3] The figures mentioned here are average realized amounts. The amounts may vary between individuals and local UI agencies.

3. DATA

3.1. *Merged Data Registers and Survey Data.* We collected information on the 393 individuals who participated in the experiment, that is, who started to collect UI benefits between August 24 and December 2, 1998, in the two experiment cities. During the observation period, unemployment rates were low and the labor market was tight.

Our data set merges three different sources of information on the sample members. The first source is the register of UI recipients for the period 1998–2004. The second source is the wage and job duration register for the same period. The third source is a survey on search effort and search behavior, held in March 1999 among the sample members.

All information on events is daily; that is, we observe the exact day of inflow into and outflow out of UI.[4] We right-censor all observations after 26 weeks of collecting UI benefits, because an individual enters a new regime of active labor market programs after being unemployed for 26 weeks (see discussion above). Among the treated, 38.5% of the unemployment spells are censored, whereas among the controls this is 39.4%. Censoring can also occur if the exit destination differs from employment (illness, prison, not accepting suitable work, and leaving the country). There is no systematic difference in how often these other exits occur in the treatment and in the control groups. With register data, the empirical analyses do not suffer from selective nonresponse or follow-up attrition from the database.

If an individual finds work within 26 weeks, we record a number of subsequent labor market outcomes: the gross wage in the first job, the length of the first job spell, the destination state after the first job spell, and the gross wage in the second job (if the destination state after the first job spell was work). Wages are deflated to obtain real weekly wages (measured in January 1999 euros). The gross minimum weekly wage is 245 euros. In our data about 5.8% of the observed wages are below the minimum wage and 44% below the observed benefits level. Among individuals with more than one observed wage, the second wage is below the first wage in 32% of the cases.

The response rate to the survey questionnaire was 33%. We manually match survey respondents to individuals in the administrative database. To match records, we use information on the month of birth, the city of residence, gender, treatment status, having collected UI benefits before, current labor market status, and day of starting collecting UI benefits. Due to item nonresponse on these variables, we only succeeded in matching 49 individuals in the treatment group and 55 individuals in the control group. Van den Berg and Van der Klaauw (2006) investigate if there was selective nonresponse and concluded that this was not the case. In the estimation of the structural model, all 393 individuals in the administrative data are used.

The survey includes questions on how unemployed workers evaluate monitoring and on which job search channels they have used in addition to subjective evaluations of satisfaction with aspects of the benefits and reemployment system. Van den Berg and Van der Klaauw (2006), using the answers to the survey, demonstrate that the unemployed workers experience monitoring as controlling instead of advisory. This is important, as the occurrence of follow-up meetings in the treatment arm suggests that treatment may include counseling as well.

The main information we take from the survey concerns the use of job search channels. Individuals were asked to report from a list of possible job search methods which methods they had actually used during their spell of collecting UI benefits. We define the *formal* search channel as the channel in which personnel advertisements at the public employment offices/local UI agency or (commercial) employment agencies are used as methods. The *informal* search channel uses open application letters, referrals by employed workers, and search through friends and relatives as search methods. The formal channel is monitored and the informal channel is not. We keep job advertisements in newspapers out of the analyses, as in our opinion this

[4] As mentioned in Subsection 2.1, UI recipients are not all full-time unemployed; for example, they may have lost only part of their working hours and still work for the remaining hours. We simply refer to the period from the start of collecting UI benefits until the end of collecting UI benefits as unemployment and to UI recipients as unemployed workers.

TABLE 1
SUMMARY STATISTICS

	Treatment Group	Control Group	p-Value for Equality
Number of individuals	205	188	
Percentage female	38.5%	41.0%	0.625
Percentage living in City 2	62.0%	59.0%	0.557
Average age	35.8	36.7	0.312
Percentage collected UI before	22.4%	26.6%	0.341
Average UI benefits	384	381	0.819
Measure of formal search	0.79 (0.12)	0.52 (0.12)	0.114
Measure of informal search	0.79 (0.15)	1.00 (0.14)	0.322
Hazard to first job	0.0434 (0.0038)	0.0419 (0.0038)	0.814
Wage in first job	413 (14)	424 (17)	0.621
Job separation hazard	0.0023 (0.0004)	0.0024 (0.0004)	0.864
Job-to-job hazard	0.0088 (0.0006)	0.0068 (0.0006)	0.026
Wage in second job	460 (25)	434 (23)	0.440

NOTES: Wages and benefits are before taxes and measured in euros on January 1, 1999. Time unit is one week. The measure of search channel usage is the used number of methods corresponding to that channel. Hazards are estimated transition rates for exponential duration distributions with covariates. Standard errors of the averages and of the estimated hazard rate levels are in parentheses.

can be formal as well as informal search—it is not clear how the unemployed worker became acquainted with the advertisement. In fact, almost all unemployed workers indicate that they examined job advertisements in newspapers.

3.2. *Summary Statistics.* In Table 1, we provide summary statistics for the variables we use in our empirical analyses. There are slightly more individuals in the treatment group than in the control group; 205 individuals received monitoring whereas 188 were excluded from monitoring. Because the registers only contain variables that are needed by the UI agency, the number of variables in this database is limited. For example, we do not have any information on occupation and level of education. The differences in gender, the city in which they live, age, level of benefits, and whether they collected UI benefits before are relatively small between both groups and never significant. Van den Berg and Van der Klaauw (2006) provide some more extensive checks for the random assignment of treatment and conclude that randomization holds. Finally, we observe if the local UI agency imposed a sanction on the UI recipient. We do not have any information on the reason why the sanction was imposed or the size and the duration of the benefit reduction. In the database, the percentage of individuals who had a sanction imposed was less than 3% among the treated as well as among the controls.[5]

Table 1 also provides statistics on outcomes. Monitored individuals use on average more formal and less informal search methods than the unemployed workers in the control group. They also have a slightly higher reemployment rate ("hazard to first job"). The level of this rate as well as of the other hazard rates is estimated conditional on covariates. Without covariates, the estimated difference of the hazard between the two groups would be biased toward zero. This is because unobserved heterogeneity tends to attenuate estimated covariate effects on hazard rates. To see this, notice that randomization upon entry into unemployment does not translate into randomization among survivors at positive durations at which hazards are evaluated (for details, see, e.g., Van den Berg, 2001).[6]

[5] Sanctions are also imposed for other reasons than lack of job search effort, for example because of not complying with administrative obligations regarding the provision of information on eligibility status.

[6] In descriptive analyses, we did not find evidence of additional unobserved heterogeneity in the reemployment rate (which would have generated negative duration dependence in the estimated rate). We suspect that this is because we focus on the first six months of unemployment and because the data consist of individuals profiled to be among the

TABLE 2
OLS REGRESSION FOR LOGARITHM OF NUMBER OF APPLICATIONS IN THE PAST MONTH

Intercept	1.303 (0.164)
Measure of formal search	0.218 (0.120)
Measure of informal search	0.157 (0.107)

NOTE: Standard errors in parentheses.

Among the monitored, the wage in their first job is on average slightly lower. Here, a caveat applies that is similar in spirit to the remark in the previous paragraph. Post-unemployment wages are only observed if they are realized before the end of the observation window. Occurrence of the latter depends both on the treatment status and on observed and unobserved individual characteristics. In any case, the difference in average observed wages pales in comparison to the standard deviations of the observed wages. As a result, the difference is not significant.

The job separation fractions in the first job are the same for both groups. Furthermore, monitored individuals are significantly more likely to make a job-to-job transition. The correlation between the wage in the first and second job is 0.8 and is similar in the treatment and control groups. The average observed wage in the second job is somewhat higher in the treatment group. However, for such descriptives of post post-unemployment outcomes, the same caveat applies as in the above paragraph.[7] Moreover, the difference in average observed second wages pales again in comparison to their standard deviations. Indeed, the observed difference is not at odds with a lower average wage in the second job among the treated versus the controls.

We measure search channel usage by the number of methods used per channel (ranging from 0 to 2 for each channel). One could argue that this is too crude a measure for search intensity. We look into this by examining the total number of job applications made in the past month as reported by those who were still unemployed at the interview date. It should be noted that this question was only answered by 46 individuals (excluding one person claiming to have made 400 applications in the past month). On average, the individuals in our sample make 6.6 applications per month with a maximum of 40. In Table 2, we regress the logarithm of the number of applications on the number of formal and informal search methods used by the unemployed worker. Use of an additional formal search method is associated with an increase in the number of applications by 22%, with a p-value of about 0.07, whereas monthly job applications are 16% higher if the unemployed worker uses an additional informal search method. Jointly, the channels have a significant impact at the 10% level.

4. STRUCTURAL ANALYSIS

4.1. *Job Search Model with Search Channels, Search Effort, Monitoring, and Job Mobility.* Our structural job search model is a sequential job search model with endogenous search effort (e.g., Mortensen, 1986), extended by allowing job offers to arrive through formal as well as informal search channels and by allowing for job search monitoring. Furthermore, we explicitly incorporate on-the-job decisions on search effort and job-to-job mobility. Our model shares some features with the model framework of Pavoni and Violante (2007).

best positioned to find work quickly. Perhaps for such individuals dynamic selection only plays a role after six months of unemployment.

[7] For example, observation of a second wage requires that the individual completes both his unemployment spell and his first job within the observation interval (i.e., within approximately six years). Since job durations tend to be longer than unemployment durations, this means in particular that the first job duration should be relatively short. This condition may be easier to fulfill if the individual was treated and, at the same time, has certain favorable individual characteristics. These characteristics may interact with the treatment status such that among those with short realized job durations, there is an overrepresentation of individuals with characteristics that are associated with a high second wage. Such an interaction effect may be due to reasons outside of the model, but it may lead the second wage to be higher among the treated if it is realized within the observation window.

Consider an unemployed worker searching for a job. This individual can search along the formal and the informal channels, which are denoted by subscripts 1 and 2, respectively. An amount of search effort $s_i \geq 0$ is devoted to search along channel i. This variable s_i, which is also called the search intensity for channel i, is to be chosen optimally by the unemployed worker. Job offers along search channel i arrive at the individual according to a Poisson process with rate $\lambda_i s_i$.

A job offer is characterized by a random draw from the wage offer distribution F_u. At the individual level, arrival times and wage offers are independent across channels, and for each channel they are independent over time. We assume that F_u is continuous with a connected support stretching to infinity, on which the density is positive. If a job offer arrives, the individual has to decide immediately whether to accept it or to reject it and continue searching.

The costs of search are expressed by the function $c(s_1, s_2)$. We require c to be increasing and convex in its arguments, with $c(0,0) = 0$. Moreover, we require $\partial^2 c/(\partial s_1 \partial s_2) \geq 0$ for $s_1, s_2 > 0$, to capture that efforts along the two channels are relatively similar activities compared to most other ways to spend time and money and to capture that a certain fraction of vacancies may be found along either channel. In the literature on search models with endogenous search effort s and a single search channel, the arrival rate and the search costs are generally taken to be proportional to s and s^2, respectively (see the survey by Mortensen and Pissarides, 1999). We require that our specification for c reduces to such a quadratic specification in case only one channel is used or in case both channels are equivalent. So, our function c has to be such that $c(s, 0)$, $c(0, s)$, and $c(s, s)$ are quadratic in s. We take the following specification:

$$c(s_1, s_2) = \left(c_1 s_1^\gamma + c_2 s_2^\gamma\right)^{2/\gamma}. \tag{1}$$

This satisfies the above requirements if $c_i > 0$ and $1 \leq \gamma \leq 2$. There are two interesting special cases of this cost function. First, if $\gamma = 1$, there is perfect substitution. In this case, the cost function simplifies to $c(s_1, s_2) = (c_1 s_1 + c_2 s_2)^2$, which means that to keep costs constant, one unit of formal search effort can always be replaced by $\frac{c_1}{c_2}$ units of informal search effort. Second, if $\gamma = 2$, the costs function equals $c(s_1, s_2) = c_1 s_1^2 + c_2 s_2^2$ so formal and informal search effort contribute additively to total costs.

The instantaneous utility of income is given by $u(w) = w$. While being unemployed, a worker received benefits b; the instantaneous utility of unemployment equals $u(b) = \kappa b$. A value for κ smaller than 1 implies that individuals dislike being unemployed relative to working at a wage equal to the benefits level. Individuals maximize their expected discounted income over an infinite time horizon. The expected discounted income (or "value of search") and the discount rate are denoted by R_u and ρ, respectively.

While being employed, individuals can search on the job. Employed workers receive job offers along the rate λs (with s the search effort in employment) and lose jobs with rate δ. A job offer is drawn from the wage offer distribution F_e satisfying the same technical assumptions as F_u. Notice that we allow the wage offer distribution to depend on the employment status. Because of our interest in the extent to which job-to-job mobility affects the effects of monitoring in unemployment, it is important to prevent that our results are biased by strong assumptions on the post-unemployment environment. As we shall see, the assumption that $F_u = F_e$ is refuted by the data.

Along this line of reasoning, we also explicitly model the search effort decision in employment. After all, both the wage offer distribution and the job search costs in employment influence the welfare loss of monitoring the unemployed. The cost of search in employment is expressed by $c_e(s)$ as a function of effort s. We are not in a position to distinguish between search channels while employed, as our data do not provide observations of indicators of search effort or the use of search channels during employment. Along the lines of the specification of the search cost function in unemployment, we assume that $c_e(s) = c_0 \cdot s^2$.

We now introduce monitoring of unemployed workers' search effort into this model. We assume that monitoring concerns only the formal search effort s_1 and not the informal search effort s_2. The local UI agency can check the number of times the UI recipient responds on a job advertisement, the number of application letters written, subscription at public employment offices, etc. It is for the local UI agency much more difficult to measure how often an individual asks friends and relatives about job openings. The monitoring effort of the local UI agency therefore focuses on search along the formal channel. Specifically, the agency imposes a minimum search effort (or threshold value) devoted to formal job search denoted by s_1^*.

Full compliance can be achieved by imposing severe punishment for noncompliance with the search requirements. In practice, the most common punishment in case of noncompliance is a sanction, which is a temporary benefit reduction. At the time of the experiment, the Netherlands had a strict sanction regime compared to many other countries (Abbring et al., 2005). The data show that sanctions are virtually absent among monitored individuals, confirming that punishments are indeed regarded as severe. We therefore simply assume that there is no noncompliance. We return to this issue in Subsection 5.3 when we compute how large punitive benefit reductions should be to ensure compliance.

Now consider how monitoring may affect the subsequent search environment in employment. In the model outlined so far, labor market outcomes after acceptance of the first job can depend on the monitoring (and on the decisions in unemployment) solely by way of the set of acceptable wage offers in unemployment. An alternative reason for a relation between the search environment and decisions in unemployment on the one hand and the search environment and decisions in employment on the other hand is that the environment in employment depends on how the individual searched in unemployment. In particular, because the search environment faced by an unemployed individual influences the way she finds her first job, it may also influence the subsequent success in climbing the job ladder.

One may consider two approaches to deal with this in the model. First, λ in employment may depend on the channel that actually delivered the first job. This would pose major problems for the empirical inference, because we do not observe how an individual found the job. We only observe the mix of channels that the individual used throughout unemployment. Barring corner solutions, any given mix may or may not cause the job to be found through the formal channel. In fact, whether the job was by coincidence found through one or the other channel may leave less of an imprint than the characteristics of the search environment that the individual faced throughout unemployment. We therefore adopt an alternative approach, in which the rate λ depends on whether the individual faced monitoring or not in unemployment. As we shall see, monitored individuals in unemployment tend to search more intensively along the formal channel than nontreated individuals. They may thus learn to deal with formal methods in an efficient way. This may be an advantage when searching for better jobs while employed. For example, writing good application letters is a skill that one may learn through experience, and this experience is obtained faster when using formal methods. On the other hand, individuals who are not monitored use informal channels more frequently, and this may foster a social network that is also useful in employment. After all, typically at most one network member is instrumental in obtaining a job, so the other members remain available for help in moving on to better jobs.[8]

4.2. *Expected Present Values and Optimal Behavior.* An employed individual losing her job believes that the state of unemployment is equivalent to the state of unemployment before the employment spell she just completed. This implies that workers who received monitoring prior to employment believe that they will receive this again, and similarly for workers who did not receive monitoring. The optimal job offer acceptance behavior of an employed worker is to

[8] This modeling approach allows monitoring to have effects even if it is not binding on the unemployed's effort, because it allows monitoring to directly affect the search effort intensity λ in employment. As we shall see, this phenomenon is empirically irrelevant, as monitoring is virtually always binding.

accept all job offers with a wage higher than the current wage. This implies that the Bellman equation for an employed worker with wage w equals

$$\rho R(w) = \max_{s\geq 0} w - c_0 s^2 + \delta(R_u - R(w)) + \lambda s \int_w^\infty (R(x) - R(w))dF_e(x),$$

where $R(w)$ is the expected present value of working in a job with wage w and R_u is the expected present value of unemployment (note that λ, $R(w)$, and R_u here depend on the exposure to monitoring in earlier unemployment). Optimal search effort s^{opt} follows from

$$s^{\text{opt}} = \frac{\lambda}{2c_0} \int_w^\infty (R(x) - R(w))dF_e(x).$$

For an unemployed person not exposed to monitoring the Bellman equation is

$$\rho R_u = \max_{s_1,s_2\geq 0} \kappa b - (c_1 s_1^\gamma + c_2 s_2^\gamma)^{2/\gamma} + (\lambda_1 s_1 + \lambda_2 s_2) \int_\phi^\infty (R(w) - R_u)dF_u(w). \tag{2}$$

The optimal strategy of an unemployed individual has a reservation wage property in the sense that she accepts a job offer if and only if the wage exceeds a reservation wage ϕ^{opt}. The reservation wage follows from $R(\phi^{\text{opt}}) = R_u$. It can be shown that in the generic case of $\gamma \neq 1$ the optimal search efforts have a unique interior solution and satisfy

$$s_i^{\text{opt}} = \frac{\lambda_i}{2c_i} \left(c_i + c_j \left(\frac{c_i}{\lambda_i} \frac{\lambda_j}{c_j} \right)^{\frac{\gamma}{\gamma-1}} \right)^{1-2/\gamma} \int_{\phi^{\text{opt}}}^\infty (R(w) - R_u)dF_u(w), \qquad i \neq j. \tag{3}$$

Now consider an unemployed individual exposed to monitoring. It is clear that if optimal formal job search effort s_1^{opt} in the unrestricted case lies above the threshold value s_1^*, then the individual will not change her behavior, so a change in s_1^* does not have any effect. We focus on the more interesting case in which the required effort is binding. The individual then complies with the formal search requirement by choosing $s_1 = s_1^*$. In this case, the optimal strategy of the unemployed worker can be summarized by ϕ^* and s_2^*.

Given the reservation wage and given s_1^*, the optimal search effort along the informal channel satisfies the first-order condition

$$2c_2(c_1 s_1^{*\gamma} + c_2 s_2^{*\gamma})^{(2/\gamma)-1} s_2^{*\gamma-1} = \lambda_2 \int_{\phi^*}^\infty (R(w) - R_u)dF_u(w).$$

This equation does not have a closed-form solution. There are, however, two interesting cases to consider. First, in case of perfect substitution ($\gamma = 1$),

$$s_2^* = -\frac{c_1}{c_2} s_1^* + \frac{\lambda_2}{2c_2} \int_{\phi^*}^\infty (R(w) - R_u)dF_u(w).$$

Here, increased monitoring is relatively ineffective due to effort substitution.[9] Second, if $\gamma = 2$, then

$$s_2^* = \frac{\lambda_2}{2c_2} \int_{\phi^*}^\infty (R(w) - R_u)dF_u(w).$$

[9] Keeley and Robins (1985) also mention the possibility of substitution of search effort in response to monitoring of the formal search channel. They do not provide a formal theoretical analysis.

Now there is no direct substitution effect on effort along the informal channel. There is, however, still an indirect effect of monitoring via the decrease in the reservation wage, which increases the optimal s_2^*. Notice that s_1^{opt}, s_2^{opt}, and s_2^* all depend on the job offer arrival rate λ in employment, which in turn depends on the monitoring regime.[10]

The optimal reservation wage ϕ^* now follows from Equation (2), where the right-hand side is now maximized over s_2 with s_1 fixed at s_1^*. Note that the marginal returns to formal job search effort are now lower than the marginal costs. The optimal reservation wage is decreasing in the binding minimum required formal search effort level. Unemployed workers are forced to behave suboptimally, so being unemployed becomes less attractive, and therefore they are willing to accept jobs with lower wages. For essentially the same reason, unemployed workers would not participate voluntarily in a monitoring scheme with a binding minimum search effort if the alternative is a monitoring scheme with a nonbinding minimum effort level. Monitoring as such (compared to no monitoring) may still be attractive if the monitoring effect on λ is positive and sufficiently large. Barring such admittedly extreme cases, the advantages of monitoring are outside of the individual's decision problem. The agency may want to reduce the total payment of UI (i.e., to increase θ by way of monitoring) because it believes that the advantages of this outweigh a reduction of the unemployed worker's present value. We return to this issue when performing policy simulations using the estimated model.

The reemployment rate θ is given by

$$\theta = (\lambda_1 s_1 + \lambda_2 s_2)\, \overline{F_u}(\phi), \tag{4}$$

with $\overline{F_u} := 1 - F_u$. Stricter monitoring causes an increase in formal search s_1, which positively affects θ. Next, depending on the value of γ, there may be a direct substitution effect on s_2, which reduces θ. Furthermore (barring again extreme cases where monitoring has a large positive autonomous effect on λ), monitoring causes a reduction in the reservation wage ϕ, which positively affects $\overline{F_u}(\phi)$. This reduction in ϕ transmits the indirect effect on s_2, which has a positive effect on s_2, which in turn has a positive effect on θ. If $\gamma = 2$, the negative direct substitution effect is absent and increased monitoring has unambiguously positive effects on the reemployment rate. For lower values of γ, the overall effect is theoretically ambiguous and depends on the relative effectiveness of the search channels.

Again barring extreme cases where monitoring has a very large positive autonomous effect on λ, its effect on the post-unemployment wage is adverse. At the individual level, the distribution function of the first accepted wage w equals $F_u(w)/\overline{F_u}(\phi)$. The lower ϕ is the lower is the average accepted wage. The mean of the subsequent job duration is decreasing in the wage w. These effects linger on in subsequent jobs. Note that the job duration also depends on λ which may depend directly on monitoring.

4.3. *Connecting Search Effort to the Usage of Search Channels.* In this subsection, we discuss the connection between the theoretical search effort s_i and the indicators of the usage of search channel i. Let n_1 denote the number of formal search methods and n_2 the number of informal search methods, with formal and informal as defined in Subsection 3.1, and with superscripts $*$

[10] It is difficult to derive comparative statics results on the signs of the effects of the baseline level of λ (i.e., of the level among nonmonitored) on search effort and the interaction effects between λ and the presence of monitoring on search effort and other model outcomes. In a simpler model with one search channel in unemployment and no monitoring, an increase of λ has two effects on search effort in unemployment. A direct effect reflects that it becomes more attractive to be employed to search further for an even better job. This increases the incentive to replace unemployment by employment, that is, to increase search effort. An indirect effect reflects that the expected present value of unemployment increases by the improved attraction of being employed. This is a reason to cut down the spending on search costs in unemployment, that is, to decrease search effort. In numerical analyses, we find that the first effect typically dominates.

and opt denoting the treatment regime. For each channel, we assume that effort is proportional to the number of methods used,

$$\alpha_1 = \frac{s_1^{\text{opt}}}{n_1^{\text{opt}}} = \frac{s_1^*}{n_1^*}$$

and

$$\alpha_2 = \frac{s_2^{\text{opt}}}{n_2^{\text{opt}}} = \frac{s_2^*}{n_2^*},$$

where α_1 and α_2 are two unknown scale parameters.

The values of the parameters $c_1, c_2, \lambda_1, \lambda_2, s_1^*, \alpha_1, \alpha_2$ can be modified in particular ways without affecting the outcomes of interest. Intuitively, this is because these parameters all relate search effort to the outcomes of interest, whereas the actual search effort levels themselves are unobserved. We deal with this by imposing the innocuous normalization $\alpha_1 = \alpha_2 = 1$, implying that effort is measured as the number of methods used.

Search effort in employment is not observed either, and neither is the number of methods used in employment. This effort affects the job exit rate (and hence job durations) only. It is not difficult to see that with optimally chosen effort, the job exit rate depends on λ and c_0 by way of λ^2/c_0 only. We thus normalize $c_0 = 1$. Note that this implies that the value of λ is not directly comparable to the values of λ_i.

4.4. *Parameters, Identification, and Covariates.* We assume that wage offers always exceed the mandatory minimum wage $w_{\min}$ and that the wage offer distributions are exponential,

$$F_j(w) = 1 - \exp\left(-\mu_j(w - w_{\min})\right), \qquad w \geq w_{\min}, \qquad j = u, e, \tag{5}$$

so the expected wage offers equal $w_{\min} + \frac{1}{\mu_j}$.

In the data, many wages are close to the mandatory minimum wage, implying that the lowest reservation wage among unemployed workers is at or below the mandatory minimum wage. This is necessary to assess the fit of the functional forms of F_j $(j = u, e)$ (Flinn and Heckman, 1982). Also, it prevents that counterfactual policy scenarios where the reservation wage drops below the lowest observed wage are driven by untestable functional form assumptions.

As a result, the unknown structural parameters in the model are $c_1, c_2, \lambda_1, \lambda_2, s_1^*, \gamma, \mu_u, \mu_e, \lambda, \delta, \kappa$, the direct effect of monitoring on λ, and the discount rate ρ. We fix the latter to 5% annually.

For expositional convenience, we discuss model identification for the case in which the data do not contain measurement errors or covariates x.[11] We start with the observation that individuals are heterogeneous in terms of b and in terms of their monitoring regime. Recall that we observe these, but we do not observe whether monitoring is binding for a given individual. From the right-hand tails of the distributions of accepted (and hence observed) wages, we identify μ_u and μ_e. Data on job durations then immediately identify λ, including the way it depends on the monitoring regime in previous unemployment. The rate δ is directly identified by the duration in employment until unemployment.

In our data, a fraction of the unemployed are observed to accept wage offers at and around the minimum wage. By implication, these must have used a reservation wage at or below $w_{\min}$. The reemployment rate θ for these individuals satisfies $\theta = (\lambda_1 n_1 + \lambda_2 n_2)$, and this enables identification of λ_1 and λ_2 from the association between n_1 and n_2 on the one hand and the unemployment

[11] The expressions for the likelihood contributions in the next subsection may serve to elucidate the discussion in this subsection.

duration on the other. This leaves us to consider the remaining parameters c_1, c_2, s_1^*, γ, and κ. A number of outcomes are informative on these. Notably, for each treatment status, we observe the means of n_1, n_2, the unemployment duration, and the post-unemployment wage. (Notice that unemployment durations and post-unemployment wages are only informative on these parameters if some of the unemployed individuals do not accept every offer.) The model equations corresponding to these eight outcomes are complicated and highly nonlinear. Moreover, for each treatment status, we can vary b in the data, and this gives an additional range of informative observations. For example, consider the set of nonmonitored individuals who accept offers at the minimum wage. This set corresponds to all values of b below a certain value that can be observed. We may then consider increasing values of b beyond this value. This must go along with higher reservation wages. We can then use observed post-unemployment outcomes by b to trace out the reservation wage as a function of b. The model equations describe how n_1 and n_2 move along.

The variation in the above-mentioned observed outcomes along with the model equations capturing individual behavior suffice to uncover the values of c_1, c_2, s_1^*, γ, and κ. The equations and numerical simulations suggest that the channel substitution parameter γ is determined by the extent to which individuals decrease their informal search effort indicator n_2 as a result of the increased formal search effort due to monitoring. That is, it is determined by the cross-regime difference between the associations of n_1 and n_2. The relative magnitudes of c_1 and c_2, in contrast, are determined by how search efforts in the absence of monitoring vary with income flow in unemployment b.[12]

As noted above, we allow some of the unknown parameters in the structural model to vary with observed individual characteristics x using parametric specifications. The job search efficiency parameters while being unemployed λ_1 and λ_2 are specified as $\lambda_i = \exp(\lambda_{0i} + x'\lambda_x)$. Thus, they have different intercepts but they share covariate effects. The parameter λ is specified analogously, that is, as a log-linear function of x with the same vector of coefficients λ_x and with the monitoring regime in previous unemployment as an additional covariate. Furthermore, $\mu_j = \exp(\mu_{0j} + x'\mu_1)$, where $j = u, e$. In other words, the log mean wage offer in excess of the minimum wage has an intercept that depends on the current labor market state, but the covariate effects are common across these states. Note that a positive sign of μ_1 is associated with a negative effect on wage offers.

The vector x consists of a binary gender indicator, a binary indicator for the city in which the experiment took place, and an age variable. We do not allow for additional covariates for two reasons. First, as we shall see, estimation of the model specification with the current number of covariates already reaches the boundary of computational feasibility. Second, recall that the population of interest is defined by the profiling procedure described in Subsection 2.2. Specifically, of four types of individuals, we examine the type that is deemed to have the most favorable characteristics. Ideally, this profiling takes account of covariate effects on individual labor market outcomes.

We also allow the minimum formal search requirement s_1^* to depend on x. This captures, for example, that the UI agencies in both cities may apply guidelines differently or that caseworkers take individual labor market prospects into account. Specifically, we take $s_1^* = \omega_0 + x'\omega_1$.

4.5. *Measurement Errors and Likelihood Function.* We estimate the model with the Maximum Likelihood Estimation method. In this subsection, we only briefly discuss the derivation of the likelihood function, as it closely resembles the derivations in the literature on structural

[12] It is interesting to contrast this to basic search models with endogenous search effort without monitoring. With additive channels (i.e., $\gamma = 2$), if effort is not observed, the parameters λ_i and c_i are not separately identified when relying on the usual outcome variables. Observing effort is thus crucial for identification. The underlying intuition is that the usual outcome variables are more sensitive to λ_i than to c_i, whereas effort along channel i is equally sensitive to each of these two parameters. And this is because the standard outcomes depend on effort along channel *i multiplied by* λ_i. It should, however, be pointed out that many interesting model features and predictions do not require separately identifiable λ_i and c_i.

estimation of search models (e.g., Eckstein and Van den Berg, 2007). When specifying the log likelihood function we allow for measurement errors both in observed wages and search effort. The introduction of measurement errors serves two purposes. First, the estimation results become less sensitive to outliers. Second, the estimated variances of the measurement errors are informative on the extent to which the observed variation in an outcome can be explained by the model; that is, they are informative on the fit of the model. For ease of presentation, we suppress covariates x and individual indices in the remainder of this subsection.

The quantification of a likelihood contribution for given parameter values always requires the calculation of the individual's optimal strategy. The latter is formulated in terms of optimal search efforts and the reservation wage. As the model does not have a closed-form solution for the optimal strategy, it must be computed numerically. Of course, when computing the optimal strategy, we take into account whether or not the unemployed worker is exposed to job search monitoring.

To proceed, we relate model to data. First, recall from the subsection on identification that we impose a normalization that effectively delivers that the actual number of used search methods per channel equals the actual search effort per channel. We allow the observed formal and informal search efforts to contain individual- and channel-specific measurement errors, according to $\widetilde{s}_i = s_i + \varepsilon_i$ (with ε_i normally distributed with mean zero and variance σ_i^2). This implies the following likelihood contribution of search efforts:

$$\ell_1(\widetilde{s}_1, \widetilde{s}_2) = \frac{1}{2\pi\sigma_1\sigma_2} \exp\left(-\frac{1}{2}\frac{(\widetilde{s}_1 - s_1)^2}{\sigma_1^2} - \frac{1}{2}\frac{(\widetilde{s}_2 - s_2)^2}{\sigma_2^2}\right).$$

Obviously, only those individuals who answered the questions on search channels in the survey (as captured by, say, $Q = 1$, as opposed to $Q = 0$) contribute to this part of the likelihood function. But when estimating the model all 393 individuals in the administrative data are taken into account since they contribute to the other parts of the likelihood function discussed below.

For unemployed workers devoting effort s_1 and s_2 to formal and informal job search, respectively, and who have a reservation wage ϕ, the reemployment hazard equals

$$\theta = (\lambda_1 s_1 + \lambda_2 s_2) \exp\left(-\mu_u(\phi - w_{\min})\right),$$

which implies the following likelihood contribution of an unemployment spell of length t_1, given the true s_1, s_2:

$$\ell_2(t_1, d_1) = \theta^{d_1} \exp(-\theta t_1),$$

where d_1 indicates whether exit to work is observed.

If the individual is observed to find work ($d_1 = 1$), the wage in the first job equals w_1 is drawn from the density $\mu \exp(-\mu_u(w_1 - \phi))$. However, this wage is unobserved, and instead we observe the wage $\widetilde{w}_1$ that contains measurement errors. Conditional on the true wage, the likelihood contribution of the observed wage in the first job equals

$$\ell_3(\widetilde{w}_1 | w_1) = \frac{1}{\sqrt{2\pi}\sigma_e} \exp\left(-\frac{1}{2}\frac{(\widetilde{w}_1 - w_1)^2}{\sigma_e^2}\right).$$

Within the first job, the transition rate to the next job is

$$\theta_w(w_1) = \lambda s_e(w_1) \exp\left(-\mu_e(w_1 - w_{\min})\right),$$

while the lay-off rate is δ. This implies that the likelihood contribution provided by the first job spell given the true w_1 is

$$\ell_4(t_2, d_w, d_u|w_1) = \theta_w(w_1)^{d_w}\delta^{d_u}\exp\left(-(\theta_w(w_1)+\delta)t_2\right),$$

where d_w indicates a job-to-job transition and d_u indicates a lay-off. If the individual moves to a new job, the new wage w_2 must exceed w_1. Notice that because of measurement errors, the observed new wage $\widetilde{w}_2$ may actually be lower than $\widetilde{w}_1$. We take the wage measurement errors to be i.i.d. across jobs for a given individual. The likelihood contribution provided by the observed wage $\widetilde{w}_2$ given w_1 is

$$\ell_5(\widetilde{w}_2|w_1) = \Phi\left(\frac{\widetilde{w}_2 - w_1 - \mu_e\sigma_e^2}{\sigma_e}\right)\mu_e\exp\left(-\mu_e(\widetilde{w}_2 - w_1) + \frac{\mu_e^2\sigma_e^2}{2}\right).$$

As a result, the total likelihood contribution can be expressed as

$$L = \ell_1(\widetilde{s}_1, \widetilde{s}_2)^Q\ell_2(t_1, d_1)\left(\int_\phi^\infty \ell_3(\widetilde{w}_1|w_1)\ell_4(t_2, d_w, d_u|w_1)\ell_5(\widetilde{w}_2|w_1)^{d_w}\mu_u\exp(-\mu_u(w_1-\phi))dw_1\right)^{d_1}.$$

The specification of the likelihood function allows for concentrating out the estimates of the variances of the measurement errors of the search effort (these only appear in ℓ_1). The maximum likelihood estimates for these parameters are the sample variances of the difference between observed efforts $\widetilde{s}_1$ and $\widetilde{s}_2$ and optimal efforts s_1 and s_2.

5. ESTIMATION RESULTS

5.1. *Parameter Estimates.* Table 3 provides the parameter estimates of the structural model. The results show that it is important to allow for heterogeneity. Ignoring covariates reduces the number of parameters by nine, whereas the log likelihood value increases by 35.4 points. A likelihood ratio test thus indicates joint significance.

The estimated search efficiency parameters of formal search (λ_1) and informal search (λ_2) are very similar, suggesting that one additional formal search method is equally likely to generate a job offer as one additional informal search method. The search efficiency parameters are higher for women, for younger workers, and for individuals living in City 1. Having been exposed to the monitoring policy has no significant effect on the efficiency of on-the-job search. Also, this coefficient is small, indicating that the monitoring policy does not have effects beyond the search for the first job. This is an important result. Apparently, the difference in the job search environment between employment and unemployment is so large that employed job seekers do not benefit from search skills learned during excess formal search in the monitoring regime. As we shall see in Subsection 5.3, this result has implications for the welfare ranking of the policy regimes.

The wage offer distribution is different in employment than in unemployment. Employed workers receive, on average, higher wage offers ($\log\mu_u > \log\mu_e$). Men, older workers, and individuals living in City 2 receive, on average, higher wage offers than their counterparts. But the coefficient of age is only marginally significant. The intercept of the search efficiency parameter for employed workers (λ_e) cannot be compared easily to the intercepts of λ_1 and λ_2 since on-the-job search effort does not have the same unit.

The main policy parameters are those associated with the minimum requirements on formal job search effort. The highest values of s_1^* are attained for younger men living in City 2 (about 1.01). However, none of the covariate effects is significant, and also jointly these effects are insignificant (p-value for joint significance is 0.30). The minimum search requirements are binding for 86% of the individuals in the treatment group. For 14%, the unrestricted optimal formal search effort s_1^{opt} is above the minimum requirements s_1^*.

TABLE 3
PARAMETER ESTIMATES FOR THE STRUCTURAL MODEL

	Estimate (s.e.)
Formal-search channel: $\log \lambda_1$: intercept	−3.294 (0.624)
Informal-search channel: $\log \lambda_2$: intercept	−3.414 (0.507)
On-the-job search: $\log \lambda$: intercept	−7.095 (0.134)
Female (in $\lambda_1, \lambda_2, \lambda$)	0.240 (0.098)
Older than 40 (in $\lambda_1, \lambda_2, \lambda$)	−0.274 (0.098)
City 2 (in $\lambda_1, \lambda_2, \lambda$)	−0.179 (0.093)
Monitoring (in λ)	0.022 (0.059)
Wage offers: $\log \mu_u$: intercept unemployed	−4.981 (0.085)
Wage offers: $\log \mu_e$: intercept employed	−5.477 (0.095)
Female (in μ_u, μ_e)	0.799 (0.096)
Older than 40 (in μ_u, μ_e)	−0.149 (0.083)
City 2 (in μ_u, μ_e)	−0.214 (0.082)
σ_ε (measurement error wages)	92.749 (6.105)
Minimum formal search requirement s^*: intercept	0.689 (0.324)
Female	−0.210 (0.267)
Older than 40	−0.168 (0.255)
City 2	0.321 (0.308)
Costs formal search $\log c_1$	3.058 (0.997)
Costs informal search $\log c_2$	2.849 (0.657)
γ	1.147 (0.184)
$\log \delta$ (job destruction rate)	−6.116 (0.142)
κ (nonpecuniary utility of unemployment)	0.484 (0.090)
Number of individuals	393
Log likelihood	−4,666.8

TABLE 4
FIT OF THE MODEL

	Data		Predictions		
	Treatment	Control	Treatment	Control	p-Value for Equality
Measure of formal search	0.79	0.52	0.76	0.53	0.098
Measure of informal search	0.79	1.00	0.77	0.97	0.235
Hazard to first job	0.044	0.042	0.045	0.042	0.233
Wage in first job	413	424	402	404	0.834
Job separation hazard	0.0023	0.0024	0.0022	0.0022	–
Job-to-job hazard	0.0088	0.0068	0.0080	0.0072	0.743
Wage in second job	460	434	480	463	0.000

NOTES: We condition on the covariates included in the structural model when computing the summary statistics for the hazards (recall Subsection 3.2). The model predictions are based only on those individuals for whom we observe the corresponding outcomes.

The formal search channel is slightly more costly than the informal search channel. The difference is not significant. Since also λ_1 and λ_2 are close to each other, search along both channels is about equally efficient. The estimate of the parameter γ equals 1.15. Therefore, the search channels are close substitutes. The model predicts that in the control group 38% of all first jobs are found through the formal search channel. In the treatment group with the job search monitoring, this is 52%.

The weekly job separation rate is only 0.0022. So the workers lose their job on average every 453 weeks. The latter reflects that the individuals in our data are the least disadvantaged among the inflow into unemployment.

5.2. *Fit of the Model.* Table 4 shows observed averages for the main outcome variables and the corresponding predictions from the estimated model over the sample members. The first two columns replicate the corresponding columns in Table 1. In order to make a fair comparison, the predictions in the third and fourth columns are based only on those individuals for whom we observe the corresponding outcomes (recall Subsection 3.2). When computing summary statistics for the hazards, we condition on the covariates included in the structural model (recall again Subsection 3.2). The final column of the table reports the p-values for the difference in model prediction between the treatment and control group over the sample members (not taking standard errors of the model parameters into account). Analogous p-values for the observed averages are in Table 1.

The predictions for search methods usage are very close to the observed average values. The difference in predicted formal search methods usage between treatment and control groups is significant at the 10% level. The reemployment hazards are quite similar to the observed hazards, and the difference between treatment and control groups in predicted reemployment rate is not significant.

The average of the first wage after unemployment is predicted to have a value that is about 3% lower than in the data, and the observed difference between treated and controls is somewhat larger than the predicted difference. The model predicts higher wages in the second job than what is observed in the data, but with a similar difference between treatment and control group, which is also significant for the predicted wages. The average second wage levels reflect the functional form of the wage offer distributions and their truncated versions (truncated from below at the first wage), as well as the distribution of measurement noise in wage data. We return to the estimated shapes of these distributions later in this subsection.

In the model, the job separation rate is identical across individuals. Its estimated value is close to what is observed in the data. The model specification imposes no difference by treatment status either. The job-to-job hazard is slightly higher for the treated, as is also the case in the data. However, the predicted difference between treatment and control groups is somewhat smaller than in the data and not significant. Note that in general the p-values in Tables 1 and 4 are in accordance with each other.

We can also use the estimated variances of the measurement errors as measure of the goodness of fit of the model. Comparing the variance of the measurement error in wages with the variance in observed wages shows that only 25% of the variance in observed wages is due to measurement error. Since the individuals in our sample are quite homogeneous in terms of current labor market status and prospects and we only allow for a limited set of individual characteristics, measurement errors are quite small. For the formal and informal job search efforts, the model explains slightly more than 55% of the variance of the observed number of search channels used by the unemployed workers.

Finally, we compare the observed distribution of first wages after unemployment with the corresponding predicted density. Figure 1 shows a kernel estimate of the density of observed first wages and the predicted density of observed first wages. The model assigns somewhat more probability mass to the left tail of the distribution.

5.3. *Counterfactual Policy Evaluations.* We use the estimated structural model for counterfactual policy simulations. Recall from Subsection 4.4 that effects of policy changes that reduce reservation wages are identified. However, assuming that the wage offer distributions F_u and F_e are invariant to policy changes means that we abstract from equilibrium effects in the counterfactual analyses.

5.3.1. *Monitoring schemes.* In Table 5, we provide the results of some policy simulations. In the first column, we describe the situation in which the UI agency does not impose any job search requirements (or implement any other policy measures). Of course, the numbers in this column coincide with the predictions from the estimated model for what we call the control group. We mainly focus on the first 26 weeks after starting collecting UI. Without job search requirements about 66% of the individuals start working within 26 weeks. The expected costs of the eligibility checks over the first six months are about 62.80 euro.

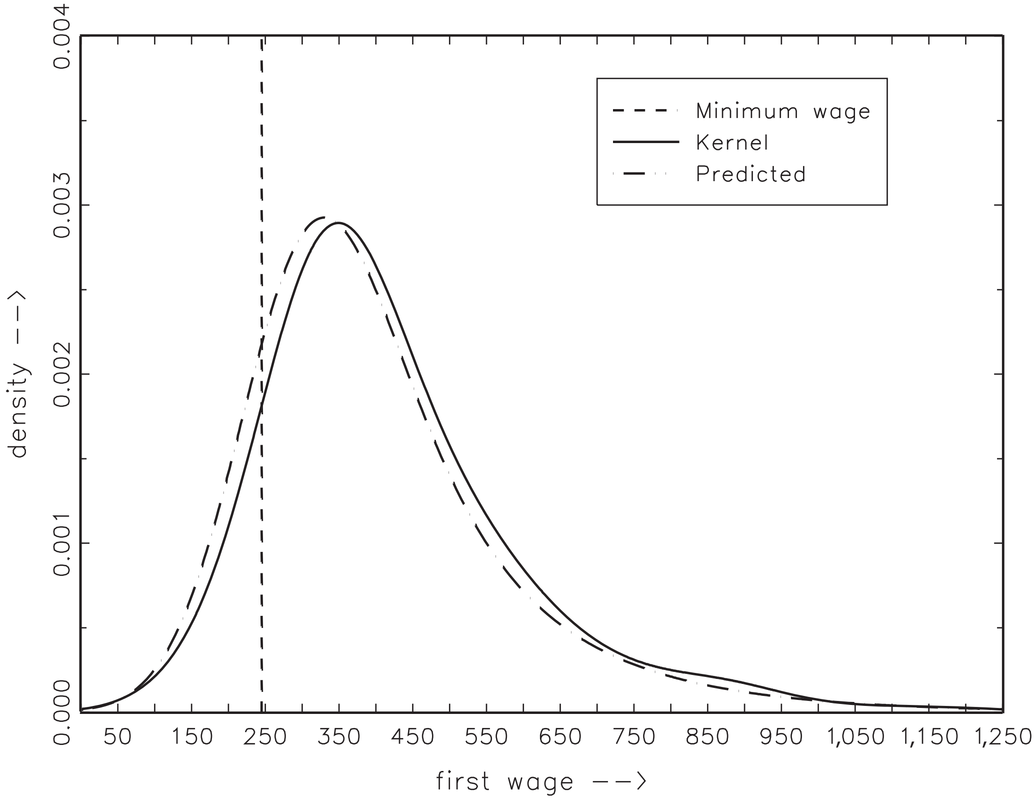

FIGURE 1

OBSERVED AND PREDICTED FIRST WAGES

TABLE 5
POLICY SIMULATIONS

	(1)	(2)	(3)	(4)	(5)
Formal search channel	0.532	0.760	2	0.536†	0.539†
Informal search channel	0.975	0.793	0.212	0.982†	0.990†
Job founds informally	61.9%	48.2%	8.4%	61.9%	61.9%
Reemployment hazard	0.0427	0.0459	0.0746	0.0433†	0.0439†
First (weekly) wage	403.45	397.55	382.88	402.71	402.67
Max{reservation wage,$w_{\min}$}	265.74	245.17	245.00	264.66†	263.69†
Present value of unemployment	543,208	545,921	534,975	543,349	543,333
Monitoring binding		86%	100%		
Agency costs	62.80	152.46	152.46	59.32	59.17
Bonus payment				98.94	106.49
Prob. work within 26 weeks	0.6608	0.6906	0.8570	0.6670	0.6670
Present value benefits	11,052	9,843	5,413	10,936	10,932
idem after 26 weeks	6,339	6,095	4,550	6,296	6,282
Sanction level (at 10% detection)		256	2,560		
"Welfare"	532,097	535,925	529,409	532,254	532,236

NOTES: "Welfare" equals the present value of unemployment minus the agency costs and the present value of benefits.
(1) No monitoring.
(2) Monitoring.
(3) Intense monitoring $s_1^* = 2$.
(4) Reemployment bonus 150 euros (effort, hazard, and reservation wage in first week reported).
(5) Decreasing reemployment bonus (start 260 euros, 10 euros decrease per week).
The reported reservation wage is the maximum of the quantity defined in Subsection 4.1 and the mandatory minimum wage.
†Due to bonuses the model is nonstationary; these are outcomes in first week of unemployment.

In column (2), we consider the case that all workers are subject to job search monitoring. These numbers coincide with the predictions from the estimated model for the treatment group. Recall that the monitoring is binding only for about 86% of the individuals in our sample. The remaining 14% of workers have unrestricted formal search effort above the minimum requirement. These individuals are still affected by the policy since this also increases job search efficiency when being employed.

Compared to the situation without job search monitoring, formal search effort is much higher and informal search effort is substantially lower. The reemployment hazard slightly increases, which also means that after 26 weeks the fraction of individuals finding work increases by three percentage points from 66% to 69%. Therefore, the present value of expected benefit payments decreases.

Because the reservation wage[13] decreases somewhat when going from column (1) to (2), the expected first wage is slightly lower in column (2). The costs of providing job search monitoring are 152.46 euro per individual entering unemployment. The monthly monitoring meetings include the eligibility checks. So providing monitoring increases the policy costs by about 90 euros. The monitoring policy has two effects. First, job search monitoring restricts unemployed workers in their search behavior, which they dislike. Second, the policy has a small positive effect on on-the-job search efficiency, which increases the value of being unemployed. Because formal and informal search efforts are found to be close substitutes with similar efficiency, the second effect dominates in the expected present value of unemployment. This increases from 543,208 to 545,921. However, note that the parameter driving the second effect (the effect of monitoring on on-the-job search efficiency) is estimated to be insignificantly different from zero. This suggests that the welfare effect of the policy is not important. To proceed, we focus on a "welfare measure" that subtracts the present value of benefits and policy costs from the expected present value of unemployment. The monitoring increases this measure from 532,097 to 535,925. The standard error of the estimated welfare measure in the absence of monitoring is 21,089. The increase in welfare is thus not significant. The increase is about 0.7% of the expected present value of unemployment, which can be considered as the measure for total consumption.[14] It should be noted that the welfare measure and the expected present value of unemployment are mostly determined by future wage earnings in employment.

Column (3) provides the results of more intense monitoring where every individual is subject to the formal search effort requirement $s_1^* = 2$. Individuals then reduce informal search effort to 0.212. The change in search effort causes the total search effort and the reemployment hazard to increase substantially. Due to this extreme monitoring, about 86% of individuals find work within 26 weeks of unemployment.

A binding minimum formal search effort forces unemployed individuals to behave suboptimally, so they would prefer to deviate from it. To ensure that they nevertheless comply with the minimum effort level, the UI agency can impose financial punishments for violations. We can use our estimated model to compute the required size of such sanctions to be sufficiently deterring to prevent violations. Since individuals are heterogeneous in characteristics and in their benefits level, the required sanction level differs between unemployed workers. Furthermore, the detection rate of noncompliance is important. In our computations we assume that if an unemployed worker does not comply, in each time unit of one week the probability of getting a sanction equals 0.1. It turns out that in order to force all unemployed workers to comply with the high formal search requirement $s_1^* = 2$, the financial punishment should be at least 2,560 euros. Furthermore, if the sanction is below 1,370 euros, no unemployed worker complies with the formal search requirements. These are large numbers in the light of the sanction magnitudes in the regime that was prevalent at the time (see Subsection 2.2). This suggests that enforcement

[13] In the remainder of the article, we simply denote the reservation wage as the maximum of (i) the reservation wage as defined in the model and (ii) the mandatory minimum wage. Clearly, both reservation wage definitions are behaviorally and observationally equivalent.

[14] Consumption-equivalent welfare can be seen as being equal to the expected present value of unemployment if the individual under consideration is a newly unemployed individual.

of such a regime requires a higher detection probability, either through a higher sampling rate of individuals by the monitoring agency or through a higher probability of observing a violation conditional on having sampled a noncompliant individual.

The sanctions above apply to the counterfactual strict monitoring regime. We can also compute the size of the sanctions for the actual monitoring policy. Recall that the minimum search requirements under this monitoring scheme are not binding for 19% of individuals, so for these unemployed workers the threat of a sanction is not necessary. To accomplish that all unemployed workers comply with the minimum formal search requirements, the size of sanctions should be at least 256 euros. This magnitude is lower than the sanction magnitudes in the regime that was prevalent at the time, which explains why (according to the data) sanctions are virtually absent in that regime. This constitutes an external validation of our structural estimation results.

5.3.2. *Reemployment bonuses.* Next, we consider policies that combat moral hazard without monitoring. In particular, we focus on reemployment bonuses and unemployment benefits reductions. We compare these policies to monitoring, invoking cost neutrality on the part of the UI agency or neutrality in terms of the expected present value of unemployment.

For reemployment bonuses,[15] we consider two alternative schemes. First, we show simulation results where an unemployed worker receives a one-time bonus when finding work within 26 weeks after becoming unemployed. The model becomes nonstationary as bonuses are tied to a specific period of being unemployed. The level of the bonus is chosen such that the expected bonus payment (and other policy costs) roughly equal the costs of providing job search monitoring. This implies that in column (4) we focus on a reemployment bonus of 150 euros. The reemployment bonus slightly increases both formal and informal search effort, and it reduces the reservation wage somewhat right at the start of unemployment. As unemployment proceeds, individuals modestly increase their search effort and lower their reservation wage, until 26 weeks. The increase in search effort is proportional, such that the fraction of jobs found informally remains unaffected. Overall, the exit rate within 26 weeks is only slightly higher due to the reemployment bonus. The reemployment bonus slightly increases welfare. Not only do the unemployed workers benefit from a higher value of search, the total costs (benefits, checking, and bonus payments) to the UI agency are slightly lower than in the no-policy case. It should be noted that we abstract from direct costs of the bonus system. This includes costs made to prevent abuse, that is, to verify that the accepted job is held for a certain amount of time.

In the second bonus scheme, individuals receive 260 euros for finding work within one week, while every subsequent week this bonus decreases by 10 euros, so that the bonus is zero after 26 weeks of unemployment. This bonus scheme shares some features of the optimal UI scheme as discussed by Hopenhayn and Nicolini (1997). In particular, accepting a job becomes less attractive each period. So, individuals are encouraged to devote more effort to search and to accept more job offers. Our simulation results show that after 26 weeks reemployment is about the same as in the previous bonus scheme, whereas the expected bonus payments are slightly higher. In the first few weeks, reemployment rates are higher than in (4), whereas toward the end of the first 26 weeks of unemployment they are lower. In terms of value of search, costs for the UI agency, and welfare, the two bonus schemes have very similar outcomes.

Because the monitoring policy also has a small effect after finding work, this policy generates higher welfare than a bonus scheme. In case the reemployment bonus increases to about 5,000 euros, the welfare in both schemes becomes similar. Such sizes are unrealistic and are also likely to affect the composition of the inflow into unemployment. We may summarize the contrast between monitoring and reemployment bonuses as follows: Monitoring generates a decrease of unemployment durations. In this process, it causes effort substitution and utility loss for the unemployed worker. However, due to job mobility, the relative size of this loss and the

[15] Experimental studies on the effect of reemployment bonuses are surveyed in Meyer (1995). See also Card and Hyslop (2005) for a more recent study. Usually the effects on reemployment are found to be positive.

long-run effects on wages are small. Monitoring is cheap for the UI agency. Replacing such a cheap monitoring system with an equally cheap reemployment bonus system is not useful: The latter exerts almost no effects.

5.3.3. *Unemployment benefits reductions.* We now compare monitoring to reductions in the benefits level b. This naturally begs the question what is the rationale behind unemployment benefits in the first place. Recall that the model is not an equilibrium model, and the existence of a UI system is not justified within the model. We do not allow for savings, and benefits simply provide an income stream such that the individual survives while out of work. We assume that benefits are funded out of taxes on labor earnings, but we do not close the model with an equilibrium budget equation. An alternative approach would be to allow for risk aversion and to include savings and assets in the model. However, such models are intrinsically more demanding from a computational point of view, and our analysis is already close to the boundary of computational feasibility. Perhaps more importantly, our data do not contain any information on assets or savings. Accordingly, our counterfactual analyses with the benefits level are primarily intended to quantify policy effects at the individual level. This in turn sheds light on the extent to which post-unemployment conditions matter for the policy effects and the average treatment effects.

For many unemployed workers participation in the monitoring policy increases the value of search because the policy has a small positive (but insignificant) effect on on-the-job search efficiency. Therefore, most workers would not be willing to accept a benefits cut to avoid the monitoring. Furthermore, those unemployed workers for whom the value of search is lower when being exposed to the monitoring scheme would on average only accept a benefits cut of about 3.6% to avoid monitoring. This confirms the anecdotal evidence that complaints about the monitoring policy among those assigned to the treatment group were absent.

We finish this subsection by examining how the effects of monitoring and a benefits reduction depend on the benefits level. In Figure 2, we show the effects of monitoring and a 20% benefit reduction on search behavior and reemployment of a representative individual as a function of the UI benefits levels. First, without any policy intervention, both formal and informal job search efforts are decreasing in the benefits level. Only for unemployed workers with high benefits does the reservation wage exceed the minimum wage. The reemployment rate is decreasing in the benefits level, and this decline accelerates if the reservation wage is higher. Job search monitoring is not binding for individuals with low benefits levels, as their formal search effort already is above the minimum search requirements. The impact of job search monitoring on formal search efforts becomes larger as the benefits level increases. This is because monitoring mainly affects individuals with low formal search effort, and these are individuals with high benefits. The substitution away from informal search also increases in the benefits level. In comparison, the benefits reduction affects individuals at all levels of unemployment benefits. As with monitoring, the effect on search effort becomes more substantial as the benefit level increases. Both job search monitoring and benefits reductions tend to depress reservation wages, and therefore reemployment rates increase in either case. In sum, the main qualitative difference is that, at low benefits levels, monitoring does not exert an influence, whereas a benefits reduction does. Notice that a proportionate decrease of the benefits level is stronger in absolute terms for individuals with high benefits levels. If the benefits decrease involves a fixed amount or if individuals use a logarithmic utility flow function, then the findings would be less dramatic.

We may summarize the contrast between monitoring and benefits reductions as follows: For the UI agency, the benefits reduction is the cheaper policy, even though the corresponding gain in reemployment rates is smaller. From an average welfare point of view, the benefits reduction seems to be the most attractive. However, implementation of a benefits scheme that depends at the individual level on whether monitoring is binding or not seems unrealistically complex. A benefits reduction across the full population is unattractive among workers with adverse labor market conditions. Therefore, a benefits reduction policy may not have political support.

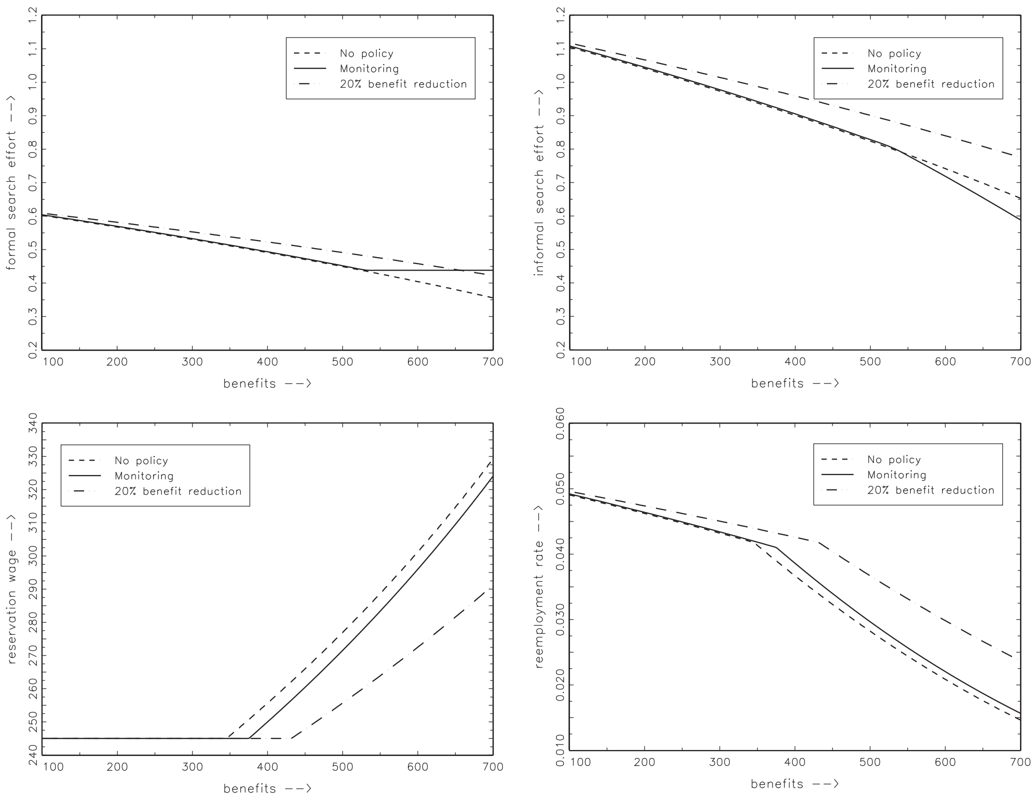

FIGURE 2

SIMULATED EFFECT OF JOB SEARCH MONITORING AND A 20% BENEFITS REDUCTION FOR A REPRESENTATIVE UNEMPLOYED WORKER

5.4. *The Importance of Job Mobility.* The labor market in the Netherlands is characterized by a high degree of flexibility and job mobility (see, e.g., Ridder and Van den Berg, 2003). Our data show a substantial degree of mobility, often associated with relatively large wage increases. Recall from Table 5 that for most unemployed workers the reservation wage equals the minimum wage. In unemployment, the strategic rejection of low wage offers to wait for better offers is not optimal compared to using the first job as a stepping stone toward better paying jobs. Thus, virtually all job offers are acceptable to the unemployed. Indeed, our results indicate that almost half of the unemployed workers even accept a wage below their benefits level.

To study the extent to which job mobility drives the results, we examine model outcomes if we rule out job mobility (and job destruction) by construction in the model. In that case, the effects of accepting lower wage offers are permanent. Table 6 gives the results. A comparison with Table 5 shows that poorer post-unemployment circumstances reduce effort through both search channels. The consequence is that the reemployment hazard drops dramatically. Indeed, the possibility of job mobility is a major determinant of the well-being and the outcomes of unemployed workers. As such it is much more important than monitoring or the other policy instruments that we considered, with the exception of the counterfactual intense monitoring regime.

Relatively speaking, monitoring is now more effective, as the weekly reemployment hazard increases by more than 50%. Without job mobility, the unemployed workers prefer a lower level of search effort in general, and therefore the substitution between formal and informal search induced by monitoring is higher. This can be seen, for example, by comparing which

TABLE 6
COUNTERFACTUAL CONDITIONS: NO JOB MOBILITY

	(1)	(2)	(3)
Formal search channels	0.207	0.750	2
Informal search channels	0.379	0.172	0.049
Job founds informally	61.9%	16.5%	2.1%
Reemployment hazard	0.0194	0.0306	0.0700
First (weekly) wage	392.59	383.38	382.71
Reservation wage	254.88	245.67	245.00
Present value of unemployment	266,696	255,293	248,043
Monitoring binding		100%	100%
Agency costs	81.05	152.46	152.46
Prob. work within 26 weeks	0.365	0.545	0.837
Present value benefits	114,211	14,189	5,794
benefits (26 weeks)	8,317	7,042	4,745
"Welfare"	152,404	240,951	242,097

NOTES:
(1) No monitoring.
(2) Monitoring.
(3) Intense monitoring $s_1^* = 2$.

fraction of the jobs is found informally. The intense monitoring (column (3)) is again efficient in stimulating reemployment.

In order to assess long-run effects on wages, it is useful to examine the present value of unemployment (i.e., the expected present value of all current and future income streams). This value now decreases upon monitoring, whereas it increases if job mobility is possible. We conclude from this that job mobility helps to counteract adverse long-run effects of monitoring on income in employment.

6. SENSITIVITY ANALYSES

In order to assess the robustness of the results, we extend the model in a number of ways. First, we extend the specification of the instantaneous utility of being unemployed by taking $u(b) = \kappa_0 + \kappa_1 b$. It turns out that the parameter κ_0 is not significantly different from 0. This parameter is even estimated to be negative, implying that individuals dislike being unemployed for nonpecuniary reasons. It should be noted that κ_1 is estimated to be somewhat higher than in the baseline model.

In a second extension, we allow the wage offer distribution of the formal and the informal channel to differ. Admittedly, such an inference is problematic in the light of the fact that we do not observe through which channel unemployed workers find a job. Therefore, if the difference between the estimated distributions is insignificant, then this should not be taken as strong evidence that they are actually close. As it turned out, and with this caveat in mind, the estimated distributions were estimated to be close to each other and the small difference was also not significant.

Finally, we allow for unobserved heterogeneity by allowing the cost function of job search effort among the unemployed to depend on unobserved characteristics. This is in line with Fougère et al. (2009), who have a "random effect" component in their cost function that may differ across individuals. We adopt a discrete distribution, effectively allowing for high unit cost and low unit cost individuals. As it turns out, during the optimization of the log likelihood function, the mass points of the distribution converge to each other, and hence the variance converges to its boundary value zero. Thus, allowing for unobserved heterogeneity in search costs does not yield a significant improvement of the model fit.

7. CONCLUSIONS

Since the monitoring policy regime is relatively cheap, this policy is cost effective for the UI agency. We find that the effect of monitoring on the job search efficiency in subsequent employment is small and insignificant. Ignoring for the moment the statistical insignificance, the point estimates predict that many workers benefit somewhat from exposure to the policy. On average, the monitoring policy is more attractive than a benefits reduction (which is cheaper for the UI agency) or the provision of reemployment bonuses. The introduction of a more severe monitoring regime has a substantial effect, but such a policy would also require substantial punitive sanctions to induce unemployed workers to comply.

On the methodological side, this article shows that a social experiment with data from a survey as well as from registers can be fruitfully used to estimate job search models that deal with nontrivial search technologies and post-unemployment outcomes. Indeed, the structural model captures the key differences in labor market behavior and outcomes between the treatment and control groups observed in the data. The structural estimation results allow for counterfactual analyses of a range of relevant alternative policy measures. This provides insights and results that cannot be obtained with reduced-form evaluations. Because of the equipoise principle, social experiments are often deemed unethical if the expected treatment effect is large. Also, social experiments are often modest in size. This makes the combination of social experiments and structural inference, where restrictions from economic theory are used to complement the empirical evidence, particularly fruitful.

We find that post-unemployment outcomes are important determinants of the effectiveness of active labor market policies for the unemployed. This is not just a matter of active labor market policies affecting post-unemployment outcomes. Rather, the conditions after unemployment influence the extent to which a policy measure exerts short-run and long-run effects. Long-run effects may be unforeseen or may be deemed irrelevant by the UI agency, since the latter is primarily concerned about total UI payments. However, they may be important for the unemployed. In the article, we focus on the role of post-unemployment job mobility on the long-run effects of monitoring. The option of job mobility affects the unemployed individuals' behavior, but it also affects the extent to which undesirable long-run effects of monitoring on wages are mitigated. We find that a high job mobility slightly reduces the short-run impact of monitoring on reemployment, but perhaps more interesting, job mobility helps to counteract adverse long-run effects of monitoring on income in employment. To put it simply, with high job mobility it does not matter so much that monitoring drives unemployed workers into a first job with a low wage. We view these results as important for policymakers. They may also serve as useful inputs for studies of optimal UI and optimal active labor market policy designs.

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